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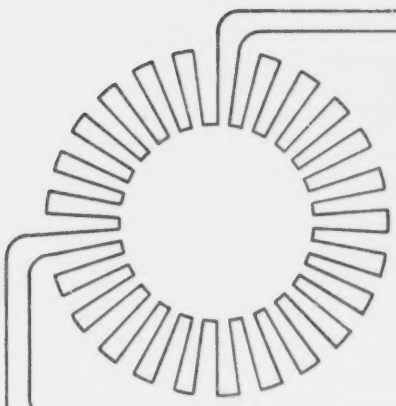

Center for Human Resource Research

Unemployment Insurance and the Duration of Unemployment

A Note on Ehrenberg and Oaxaca

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There is by no means a precise estimate of the influence of Unemployment Insurance (U.I.) on the duration of unemployment even though a variety of studies of this issue have been completed in the last decade. Hamermesh summarizes a dozen studies and concludes that his best estimate of the influence of U.I. is that the duration of an individual's unemployment will be prolonged by about one-half a week in response to a 10 percent increase in the ratio of U.I. weekly benefits to the individual's average weekly earnings prior to unemployment. He is careful to state that "this is not an exact figure, but it does appear that there is some effect, certainly above zero and probably less than one week. A more precise estimate awaits a study that embodies the best features of all those (reviewed)."[1]

One of the more interesting studies, by Ehrenberg and Oaxaca (E+O), was reported earlier in this journal.[2] The study utilized the four age-sex specific cohorts of the National Longitudinal Surveys (NLS) to estimate a linear model with duration of unemployment over a one-year period as the dependent variable. That study, using ordinary least squares estimation, found that the impact of increasing the wage replacement ratio 10 percentage points (from 0.4 to 0.5) ranged from 0.2 weeks added duration for young men to 1.5 weeks for older men.

Since the Ehrenberg and Oaxaca study was completed, new data have become available for the NLS which allow us to test the sensitivity of the earlier results. In this note we report what occurs when the original E+O specifications are changed. We find that when the model has been extended to reflect

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[1] Hamermesh (1977), p. 37.

[2] Ehrenberg and Oaxaca (1976).

differences between those eligible and those not eligible to receive U.I., the strong relationship between the wage replacement ratio and duration of unemployment disappears.

Making distinctions about U.I. eligibility is important. Given the broad coverage of the U.I. program at present, the question important for policy purposes is not whether those eligible differ significantly from those ineligible in terms of the duration of unemployment they experience. Rather, the question is whether those eligible for U.I. behave in a different manner when they receive larger and larger weekly U.I. benefits.

Ehrenberg and Oaxaca include in their estimates both the effects of eligibility on duration and the effects of varying weekly amounts of U.I. on duration, since an unknown number of respondents who did not receive any U.I. benefits are included in the analysis. For these respondents, the wage replacement ratio is zero.

Zero values for the replacement ratio can lead to biased results of the effect of U.I. on the duration of unemployment in two ways. 1) Some respondents may be eligible for U.I. but not apply. Those who expect a short duration of unemployment will be less likely to apply for U.I. even though eligible. Thus short duration of unemployment is associated with the zero replacement ratio which was assigned to the respondent because U.I. was never received. The correlation between a zero replacement ratio and short duration makes the relationship between positive replacement ratios and longer duration appear to be stronger, thus overstating the effect of U.I. on duration. 2) Respondents who report zero receipt of U.I. may have been ineligible for U.I. One of the major kinds of disqualification is voluntary separation from a job. Voluntary quits could be associated with short

duration of unemployment as follows: a respondent may expect short duration since good job prospects in the local labor market were the inducement to quit. If job prospects were bad, why quit? If we control well for local labor market conditions, voluntary quits would not necessarily bias the estimates. If local labor market controls fail (omitted variables), a correlation is established between voluntary quits with their corresponding zero-valued replacement ratios and short duration. Again, the effect of positive replacement ratios on duration is overstated.

In the present analysis, each of the potential biases indicated above is tested using data from the NLS files for young and older men in 1970 and 1971. The E+O study used earlier information for men and women from 1966 to 1969. Complete work histories exist for the more recent years, yielding important advantages. Unemployment can be measured by spell; reason for a job separation can be determined; and since the work histories extend over a two-year period, long spells of unemployment which occur near the interview date are not all excluded from the analysis.

The strategy for testing the sensitivity of the E+O results is three-fold. As a first step, for each male respondent the first spell of unemployment occurring in the two-year work history is identified. Using that spell of unemployment we calculate across all respondents a percentage distribution for the length of the spell identified. Separate distributions are calculated for recipients and nonrecipients of U.I. Comparing the separate distributions provides a first indication of whether including both recipients and non-recipients in an estimating model may provide unreliable estimates of the relationship between U.I. and duration.

As a second step, duration equations similar to those used by Ehrenberg and Oaxaca are estimated for male job changers experiencing a spell of unemployment in 1970 or 1971, and results are compared with the earlier reported E+O analysis. Finally, the E+O equations are tested for sensitivity to changes in specification. Those ineligible for U.I. are identified and excluded from the sample. Eligible respondents are used to reestimate the E+O equations, making adjustments in the replacement ratio for those who, though apparently eligible for U.I., did not report receiving benefits. The newly estimated equations are compared with the replication equations to assess the model's sensitivity.

Results

We do find a difference in the percentage distributions for the length of a spell of unemployment. Table 1 shows the comparative percentage distributions for those reporting receipt of U.I. and those not reporting U.I.

INSERT TABLE 1 ABOUT HERE

The distributions differ for respondents with duration longer than 26 weeks reflecting the influence of state laws regarding maximum duration. But more importantly, the distributions also diverge for respondents whose spells of unemployment are quite short in duration. The distributions for nonrecipients of U.I. contain proportionately more respondents with quite short durations of unemployment. The divergence in distributions may mean that those ineligible for U.I. behave differently than those eligible, but it may also mean that reported receipt of U.I. is <u>not</u> a good proxy for eligibility. If the latter is true, then some respondents with short duration of unemployment are actually

eligible for U.I. and should be included in the U.I. distributions instead of the non-U.I. percentage distributions shown in Table 1.

Results from the percentage distributions support the strategy for testing the sensitivity of the E+O models, both for effects introduced by ineligible respondents and for effects of respondents who, although eligible, do not report receipt of U.I.

For predicting U.I. eligibility we construct the following model:

$$E = \alpha_0 + \alpha_1 Q + \alpha_2 U + \alpha_3 I + \alpha_4 J + \alpha_5 W + \alpha_6 P \qquad (1)$$

where

E = a dummy variable coded one when the respondent reported receipt of U.I. in the same year that a spell of unemployment occurred.

Q = a dummy variable coded one if the respondent quit voluntarily from the job held immediately prior to a spell of unemployment.

U = a dummy variable coded one if the job prior to a spell of unemployment was not covered by U.I. -- salespersons in real estate and insurance, private household workers, and farmworkers.

I = a dummy variable coded 1 if wage credits in the calendar year prior to a spell of unemployment were 0-$1500.

J = a dummy variable coded 1 if wage credits in the calendar year prior to a spell of unemployment were $1501-$2000.

W = a dummy variable coded 1 if weeks of work in the year prior to a spell of unemployment were less than 28.

P = a continuous variable measuring the number of previous survey years in which U.I. was reported divided by the number of surveys prior to 1970 when the respondent was interviewed.

Some experimentation was done with various categorical variables before the variables that are shown were selected. Due to very low significance levels, J was ultimately dropped from the estimating equations for older men and W was dropped for the young men. Information for determining eligibility was taken from the first full year of data preceding the year in which a spell of unemployment was identified. Results for the estimation of E are shown in Table 2.

INSERT TABLE 2 ABOUT HERE

For both young and older men, predicted values ($\hat{E}$) were compared with the set of components used in the prediction. For both age cohorts at predicted values less than 0.3 those ineligible for U.I. were clearly excluded. The equation appeared to distinguish most clearly the eligibles from ineligibles for older men when a cutoff of 0.3 was used. Out of 147 older men who experienced some unemployment in 1970 or 1971, 44 were defined to be ineligible for U.I., using this criterion (30 percent of our sample). Four of the 44 had reported receipt of U.I. even though our criteria would indicate that they were ineligible. This error in assignment amounts to 7 percent of the total U.I. recipients. If the cutoff for $\hat{E}$ were raised from 0.3 to 0.4, 21 respondents who reported receipt of U.I. would be defined as ineligibles, 35 percent of the U.I. recipients contained in our sample. Clearly the higher cutoff point would err by defining too many respondents as ineligible for U.I.

For young men, 165 of the 348 respondents experiencing unemployment in 1970 and 1971 were defined as ineligible, again using a 0.3 cut point for $\hat{E}$ (47 percent of the sample). Of those defined as ineligible under this rule 21 actually reported receipt of U.I., 18 percent of the young male U.I. recipients. A higher cut-off at 0.4 would result in 27 percent of the U.I. recipients misclassified as ineligible to receive benefits. On the basis of these results we define any respondent with a value for $\hat{E}$ of less than 0.3 as ineligible for receipt of U.I. Those with values greater than or equal to 0.3 are defined as eligible.

The next stage of the analysis is to replicate the E+O results on the new data from 1970 and 1971. We then test the model's sensitivity when re-estimated, taking into account the problems caused by combining both eligible and ineligible respondents in the group to be used in making the estimates. The replication utilizes an equation quite similar to the original specification utilized by Ehrenberg and Oaxaca. Some new variables were added to the model, however, and a consistent set of variables was used for both young and older men. In our estimating equation the natural log of the duration of a spell of unemployment is a function of the wage replacement ratio "F" (average weekly U.I./average weekly earnings in the job immediately prior to a spell of unemployment), plus a series of control variables to proxy for the costs and benefits of prolonged search for new employment.

$$ln(D) = \beta_0 + \beta_1 F + \sum_{j=2}^{k} \beta_j X_j \qquad (2)$$

The control variables used were the following: dichotomous variables were used for race (whites = 1), home ownership, health condition which limits work, year in which the spell of unemployment occurred (occurring in the first year of the two year period = 1), and marital status (married = 1). Continuous variables were the natural log of weekly wages earned prior to a spell of unemployment, highest grade completed in school, level of assets, tenure (in months) on the job held prior to unemployment, local labor market size, local unemployment rate, income from other family members (for young men, wife's income only), number of dependents, and respondent's score on a 44-point version of the Rotter measure of internality/externality. (A respondent with a lower I/E score tends to feel that what happens to him is more due to his own doing than to fate or chance.)

Equations were estimated using ordinary least squares, results of which are shown in Tables 3 and 4. In the tables the estimated coefficients for the wage replacement ratio are compared with Ehrenberg and Oaxaca's earlier results.

INSERT TABLES 3 AND 4 ABOUT HERE

As in the earlier study, the coefficients estimated for the replacement ratio are significantly positive and relatively insensitive to the addition of control variables to the model. The significance of the replacement ratio in the young men's equation is in fact extraordinarily high. When evaluated, the coefficients for young men and for older men alike yield approximately the same impact, an estimate which is midway between the estimates derived by E+O for each of the two male cohorts. Our estimates show that the impact of an additional 10 percentage point increase in the replacement ratio is a 0.9 week prolongation of unemployment. It is curious that the estimates do not show more of a difference in impact for two groups of men who differ so widely in age.

Further comparison of the replication equations shows a consistently longer duration of unemployment for respondents in larger population centers, shorter duration for those with more dependents, and longer spells for those with more education. Younger men show a significant positive relationship between wages and unemployment, opposite that predicted by search theory. Race is a significant factor for young men but not for older men. Older men, who are more external and who have sources of income other than their own wage and salary income, have longer spells of unemployment.

The estimated results for the replacement ratio are both reasonable and highly significant, but are they robust? We have argued that respondents who have zero values for the replacement ratio may bias our results. Thus the first disturbing finding from the replication equations is the very large number of respondents reporting no receipt of U.I. (and therefore assigned a zero value for the replacement ratio). In our sample of male job changers experiencing a spell of unemployment, 67 percent of the young men and 58 percent of the older men did not report receipt of U.I. in the same year that they experienced a spell of unemployment. We assume that the same was true of the earlier E+O sample since the same criteria were used for sample selection.[3]

We found earlier, however, that a large number of our respondents had insufficient wage credits to gain eligibility for U.I. or were ineligible for other reasons. Of the young men, 47 percent were defined as ineligible by our criteria, and for the older men, 29 percent were ineligible. Excluding these ineligibles still leaves significant numbers of young men reporting no U.I., and this fact must be addressed in our sensitivity tests.

There are several possible explanations for the existence of respondents who do not report U.I. even though they are defined as eligible to receive it. Some respondents eligible for U.I. may not have filed for benefits. Alternatively, some respondents may have received U.I. but did not report it

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[3]The criteria excluded new entrants to the labor force since the sample was restricted to those employed both at the beginning and at the end of the two year period. Thus, the large number of nonrecipients of U.I. should not be due to respondents entering the labor force with no wage credits.

to NLS interviewers. In either case, the appropriate variable for measuring the impact of U.I. on their behavior is not the zero-valued replacement ratio they have been assigned but rather some measure of the amount of U.I. they could have received (for eligible nonfilers) or actually did receive but did not report. An instrumental variable is needed to approximate this unobserved variable.

Construction of an instrumental variable in this case is quite difficult, given the complexity introduced by separate state jurisdictions and formulas for calculating U.I. benefits. Instead we utilize the less complicated though less elegant procedure of substituting mean values of the replacement ratio for respondents who were previously assigned zero values and then re-estimate the equations reported in Tables 3 and 4. Using this procedure, variation in the values of the replacement ratio for respondents who do report receipt of U.I. should capture the effect of varying amounts of U.I. on duration.[4]

The results of this test for sensitivity are reported in Columns 1 and 2 of Table 5. They are not encouraging if our objective is to isolate a firm estimate of the influence of U.I. on duration. In fact, we cannot establish

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[4]A difficult question is what value should be used to approximate the replacement ratio for respondents who have unobserved values for the ratio. The mean value of the ratio appears to be a neutral choice if respondents who do not have observed values for U.I. possess the same characteristics as respondents who do have observed values for U.I. We have argued earlier, however, that the "unobservables" do differ from the "observables." Furthermore, these differences could be correlated with the wages that respondents receive. (For example, voluntary quits could have higher wages.) Thus one could argue that the most appropriate replacement ratio for "unobservables" is one where we divide the mean U.I. benefit by each unobservable's own wage (the wage is reported). The result is largely the same as the simpler procedure we utilize, however, since U.I. benefits themselves are calculated as a percentage of prior earnings. Only for respondents whose wages qualify them for the maximum benefit amount would our procedure differ substantially from the latter one.

that U.I. has any significant impact on duration. Both t-values and the estimated coefficients for the replacement ratio are reduced drastically.

In the newly estimated models, local labor market and time variables assume a more important role in predicting duration than does the replacement ratio. Size of the local labor market and the local unemployment rate are positively associated with duration even though t-values are quite low for the latter variable. Assuming more significance in equations of Table 5 is

INSERT TABLE 5 ABOUT HERE

a variable indicating when a spell of unemployment occurred. Holding constant the rising local unemployment rates in 1970 and 1971, a spell which was initiated in 1970 should on average be longer than one initiated in 1971, since our sample includes only those employed again at the 1971 interview. This result emerges in these estimates even though it was absent in the replication equations. Other significant determinants of duration for the older men were the amount of income sources other than own wage and salary income and the number of dependents. Though not quite significant, the log of prior weekly earnings was negative for older men as search theory would predict. Variables measuring assets and exogenous income had "wrong" signs in the young men's equations, undoubtedly due to the measures used as proxies.[5]

The method we have used for adjusting replacement ratios places considerable weight on 1) the variation in replacement ratios for respondents who report receipt of U.I., and 2) the method for predicting eligibility. As

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[5] A measure of assets is available only in 1966 and 1971 for young men. We used 1971 as the more current measure but for many, the measure follows rather than precedes a spell of unemployment, introducing the potential for reverse causality in the equation. The measure for exogenous income in the young men's sample was wife's wage and salary income, introducing more collinearity between this variable and marital status than was the case for older men where additional sources of income could be included.

for variation in the replacement ratios, it is not small even when zero values are omitted. For young men values ranged from 2 to 230 percent and for older men from 16 to 95 percent. In each age group, two-thirds of the replacement ratios fell outside the 40-60 percent range so there was likewise considerable dispersion of the ratios around mean values.[6]

The method for predicting eligibility is more open to challenge. Of those defined as eligible, 50 percent of the young men and 46 percent of the older men did not report receipt of U.I. It is doubtful that all of these respondents either did not report receipt of U.I. or were eligible nonfilers. More probably, some factors in determining eligibility have not been adequately measured and we have overestimated the group of U.I. recipients defined as eligible. To gather indirect evidence of this potential problem, we divided "eligible" young men into two groups--those reporting receipt of U.I. and those not reporting U.I. We then compared the two groups on several different dimensions: race, size of local labor market, Rotter score, student status, and wages, weekly earnings, occupation, industry and the prior year's weeks worked on the job prior to the spell of unemployment experienced. Results are shown in Table 6. Eligible nonrecipients of U.I. tend to be lower paid, are more likely to be black or students, and are employed disproportionately in trade or service industries.

INSERT TABLE 6 ABOUT HERE

The larger proportion of students among those whom we have classified as eligible but who reported no U.I. suggests that some respondents may be students ineligible because they were unavailable for full-time work. Nevertheless,

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[6]Mean replacement ratios were 55 percent for the young men and 41 percent for the older men.

a significant number of students do report U.I. so including all students in the "ineligible" group could not be justified.

A second possibility is that some of the eligible nonrecipients are self-employed or working in small retail or service industries not covered by U.I. We were able to establish that no respondents were self-employed; but no data were available on the size of the establishments in which the respondents worked.

Analysis of eligible nonrecipients did not yield additional possibilities for improving the prediction of U.I. eligibility. Thus as a final test of sensitivity, we have made an extreme assumption regarding eligibility. We assume that only those respondents who report U.I. are eligible. We re-estimate the E+O equations only for those reporting receipt of U.I. Given the relatively small sample size, this test allows us to determine whether the insignificance of the replacement ratio in the previous equations was simply due to the small number of respondents who actually reported U.I. With the young male sample a stepwise estimating procedure was used to eliminate the least significant variables. After entering the replacement ratio first, the only variables reported were those which retained a t-value greater than 1. Results are shown in column 3 of Table 5. In this final test, the replacement ratio loses its significance altogether, even though other variables such as health and number of dependents are significant.

Conclusions

What appear as quite strong effects of U.I. on the duration of unemployment do not persist once adjustments are made for differences in eligibility

of respondents. We conclude that for male job changers, the amount of previous wages replaced by U.I. has little effect on duration. Local labor market conditions and personal considerations such as additional sources of income and number of dependents appear to be more important than U.I.

The large disparity in results before and after accounting for differences in eligibility demonstrates the need for caution in interpreting any results of the influence of U.I. on unemployment. The disparity also shows how a considerable range of effects can be estimated where samples of observations include varying numbers of respondents who are either ineligible for U.I. or eligible for U.I. and do not report it. We need to know considerably more about the number and characteristics of individuals who though eligible for U.I. do not file. Furthermore, if survey data are to be used in estimating the impact of U.I., better information is needed to predict eligibility. Once eligibility has been controlled, the important policy question can be answered--namely, how responsive is individual behavior to changes in the liberality of U.I. benefits.

Table 1 Percentage Distributions of Unemployment Duration by Receipt or Nonreceipt of U.I.

Duration in weeks	Young men		Older men	
	Nonrecipients	Recipients	Nonrecipients	Recipients
1	17	2	15	8
2	22	4	10	8
3	10	4	5	3
4	10	11	10	3
5	8	11	6	2
6	3	4	3	3
7-12	14	24	20	16
13-18	7	11	9	19
19-24	3	12	7	10
25-30	4	4	7	13
31+	2	13	8	15
Total percent	100	100	100	100
Number of respondents	234	114	87	61

Table 2 Equations Estimating U.I. Eligibility

	Older men	Younger men
Constant	.349	.474
Q - Voluntary quits	-.195 (2.35)	-.212 (4.39)
U - Uncovered workers	-.161 (1.07)	-.134 (1.32)
I - Wage credits $0-1500	-.117 (5.56)	-.310 (5.63)
J - Wage credits $1501-2000	-	-.161 (2.25)
W - Weeks worked < 28	-.238 (2.23)	-
P - Percent of previous survey years report U.I.	.013 (6.97)	.008 (3.98)
Standard error of Y.X	0.41	0.42
F	13.76	17.11
Adj R^2	0.30	0.18

Table 3 Replication of Ehrenberg and Oaxaca Young Men

	E+O	Replication	
	Coefficient (t-value)	Coefficient (t-value)	Mean Value (Standard Dev.)
Constant	1.625	-.310	
Rep ratio	.538 (2.1)	1.181 (5.50)	.161 (.29)
Ln wages	.111 (1.1)	.292 (2.13)	5.38 (.45)
Assets (in 1,000s of dollars)	-.022 (0.7)	-.026 (1.36)	.715 (3.33)
Asset dummy	-.045 (0.3)	-	
Tenure	-.036[a] (0.9)	-.010[b] (1.25)	5.61[b] (6.76)
Labor market size (in 1,000s)	.000 (0.0)	.123 (2.36)	639 (105)
Local Unemployment Rate	.060 (1.9)	.014 (0.49)	6.02 (2.09)
Spell in 1968	.065 (0.6)	-	
Spell in 1969	-.029 (0.3)	-	
Spell in 1970		.038 (0.29)	.33 -
Exog. Income (in 100s of dollars)		-.001 (.12)	6.11 (15.79)
Race-white	-.074 (0.8)	-.345 (2.82)	.66 -
Dependents	-	-.120 (2.04)	.66 (1.15)
Married	-.252 (2.1)	.038 (0.24)	.38 -

Table 3 (continued)

	E+O	Replication	
	Coefficient (t-value)	Coefficient (t-value)	Mean value (standard dev.)
Owns home	-	.113 (0.54)	.11 -
Health limitation	-.125 (1.0)	.173 (0.82)	.07 -
Education	-.057 (2.4)	.030 (1.24)	11.67 (2.51)
Knowledge of world of work	.002 (0.3)	-	
Rotter I-E	-	.002 (0.22)	23.46 (5.24)
Mean of the dep. variable, Ln spell of unemployment	-	-	1.68
Sample size	464	348	348
R^2	.05	-	
$\bar{R}^2$	-	.14	
Stand. error of Y.X	-	1.00	
F ratio	-	4.74	

[a]Measured in years.

[b]Measured in months.

Table 4 Replication of Ehrenberg and Oaxaca Older Men

	E+O	Replication	
	Coefficient (t-value)	Coefficient (t-value)	Mean value (Standard dev.)
Constant	1.078	1.477	
Rep ratio	1.653 (2.6)	.870 (2.33)	.181 (.28)
Ln wages	.054 (0.4)	-.087 (0.34)	5.65 (.50)
Assets	.007 (0.8)	-.007 (0.99)	10.67 (16.24)
Asset dummy	-.616 (1.6)	-	
Tenure (in months)	-	.001 (0.76)	60.13 (96.31)
Years to retirement	.013 (0.5)	-	
Labor market size (in 1,000s)	-.250 (2.0)	.244 (2.28)	615 (102)
Local unemployment rate	.076 (1.0)	.018 (0.34)	5.28 (1.88)
Spell in 1970	-	.140 (0.64)	.66 -
Exog. Income	.706[a] (1.6)	.018[b] (1.32)	2.44[b] (8.17)
Race-white	.099 (0.3)	-.049 (0.20)	.68 -
Dependents	-.013 (0.2)	-.081 (1.43)	1.47 -
Married	.097 (0.2)	-.253 (0.89)	.81 -
Owns home	.259 (0.8)	-.086 (0.38)	.61 -
Health limitation	-	.090 (0.40)	.30 -

Table 4 (continued)

	E+O	Replication	
	Coefficient (t-value)	Coefficient (t-value)	Mean value (Standard dev.)
Education	-	.038 (1.16)	8.78 (3.88)
Rotter I-E	-	.026 (1.39)	24.34 (5.77)
Mean of the dep. variable, Ln spell of unemployment	-	-	2.07
Sample size	39	148	
R^2	.36	-	
$\bar{R}^2$	-	.09	
Stand. error of Y.X	-	1.16	
F ratio	-	1.97	

[a]Measured in 1,000s of dollars.

[b]Measured in 100s of dollars.

Table 5 Tests for Sensitivity

	Reestimation excluding non-eligibles and replacing zero values with mean replacement ratio		Reestimation on U.I. recipients only
	Young men	Older men	Young men
Constant	.775	4.27	1.370
Rep ratio	-.165 (.41)	.019 (.03)	.138 (0.47)
Ln wages	.095 (.41)	-.528 (1.54)	-
Assets (in 1,000s of dollars)	-.022 (.92)	-.003 (.34)	-
Tenure (in months)	.004 (.28)	-.0004 (.31)	-
Labor market size (in 10,000s)	.200 (2.35)	.340 (2.86)	-
Local ump. rate	.037 (.96)	.007 (1.10)	-
Spell in 1970	.400 (2.25)	.293 (1.08)	-
Exog. income (in 100s of dollars)	-.007 (1.30)	.026 (1.80)	-.001 (1.82)
Race-white	-.217 (1.13)	-.093 (.30)	-
Dependents	-.120 (1.35)	-.166 (2.30)	-.277 (2.60)
Married	.117 (.54)	-.14 (.37)	.327 (1.47)
Owns home	.207 (.76)	-.246 (.84)	-
Health limitation	.311 (1.01)	.068 (.25)	.713 (1.93)

Table 5 (continued)

	Reestimation excluding non-eligibles and replacing zero values with mean replacement ratio		Reestimation on U.I. recipients only
	Young men	Older men	Young men
Education	.041 (1.10)	.054 (1.22)	.039 (1.05)
Rotter I-E	-.006 (.41)	.006 (.29)	.021 (1.37)
Mean of the dep. variable, Ln spell of unemployment	1.864	2.165	
Mean replacement ratio	.518	.428	
Sample size	183	104	114
$\bar{R}^2$	.05	.13	.08
Stand. error of Y.X	1.08	1.14	.87
F ratio	1.58	2.08	2.46

Table 6 Comparative Characteristics, Young Men Defined as Eligible for U.I.

	Eligible nonrecipients	Eligible recipients
Percent nonwhite	41	19
Size of local labor market (in 1,000s)	528	680
Rotter I-E	24	23
% students in year received U.I.	23	11
Weeks worked-year prior to spell	45	46
Wage-job prior to spell	$2.43	$3.18
Weekly earnings-job prior to spell	$91	$127
One digit occupational distributions		
1 Professional, technical, 2 managerial	11%	9%
3 Clerical and kindred	12	11
4 Sales	14	18
5 Craftsmen and kindred	1	4
6 Operatives and kindred	38	44
7 Private household	-	-
8 Service workers	8	1
9 Farmers	-	-
10 & 11 Laborers	16	13
One digit industry distribution		
1 Agriculture	2%	1%
2 Mining	1	-
3 Construction	19	25
4 Manufacturing	30	49
5 Transportation	3	1
6 Wholesale/retail trade	20	11
7 Finance, insurance, real estate	1	-
8 Business and repair services	10	8
9 & 10 Personal services, entertainment	2	2
11 Professional and related services	11	2
12 Public administration	1	1

REFERENCES

Ehrenberg, Ronald G. and Oaxaca, Ronald L. "Unemployment Insurance, Duration of Unemployment and Subsequent Wage Gain." American Economic Review 66 (December 1976).

Hamermesh, Daniel S. Jobless Pay and The Economy. Baltimore, Md.: Johns Hopkins University Press, 1977.

A third area of research in which the Center has been active is manpower planning both in the U.S. and in developing countries. A current project for the Ohio Advisory Council for Vocational Education seeks to identify and inventory the highly fragmented institutions and agencies responsible for supplying vocational and technical training in Ohio. These data will subsequently be integrated into a comprehensive model for forecasting the State's supply of vocational and technical skills.

Another focus of research is collective bargaining. In a project for the U.S. Department of Labor, staff members are evaluating several current experiments for "expedited grievance procedures," working with unions and management in a variety of industries. The procedural adequacies, safeguards for due process, cost and timing of the new procedure are being weighed against traditional arbitration techniques.

Senior staff also serve as consultants to many boards and commissions at the national and state level. Recent papers have been written for the Joint Economic Committee of Congress, The National Commission for Employment and Unemployment Statistics, The National Commission for Manpower Policy, The White House Conference on the Family, the Ohio Board of Regents, the Ohio Governor's Task Force on Health, and the Ohio Governor's Task Force on Welfare.

The Center maintains a working library of approximately 9,000 titles which includes a wide range of reference works and current periodicals. Also provided are computer facilities linked with those of the University and staffed by approximately a dozen computer programmers. They serve the needs of in-house researchers and users of the National Longitudinal Survey tapes.

For more information on specific Center activities or for a copy of the Publications List, write: Director, Center for Human Resource Research, Suite 585, 1375 Perry Street, Columbus, Ohio 43201.

The Center for Human Resource Research

The Center for Human Resource Research is a policy-oriented research unit based in the College of Administrative Science of The Ohio State University. Established in 1965, the Center is concerned with a wide range of contemporary problems associated with human resource development, conservation and utilization. The personnel include approximately twenty senior staff members drawn from the disciplines of economics, education, health sciences, industrial relations, management science, psychology, public administration, social work and sociology. This multidisciplinary team is supported by approximately 50 graduate research associates, full-time research assistants, computer programmers and other personnel.

The Center has acquired pre-eminence in the fields of labor market research and manpower planning. The National Longitudinal Surveys of Labor Force Behavior have been the responsibility of the Center since 1965 under continuing support from the United States Department of Labor. Staff have been called upon for human resource planning assistance throughout the world with major studies conducted in Bolivia, Ecuador and Venezuela, and recently the National Science Foundation requested a review of the state of the art in human resource planning. Senior personnel are also engaged in several other areas of research including collective bargaining and labor relations, evaluation and monitoring of the operation of government employment and training programs and the projection of health education and facility needs.

The Center for Human Resource Research has received over one million dollars annually from government agencies and private foundations to support its research in recent years. Providing support have been the U.S. Departments of Labor, State, and Health, Education and Welfare; Ohio's Health and Education Departments and Bureau of Employment Services; the Ohio cities of Columbus and Springfield; the Ohio AFL-CIO; and the George Gund Foundation. The breadth of research interests may be seen by examining a few of the present projects.

The largest of the current projects is the National Longitudinal Surveys of Labor Force Behavior. This project involves repeated interviews over a fifteen year period with four groups of the United State population; older men, middle-aged women, and young men and women. The data are collected for 20,000 individuals by the U.S. Bureau of the Census, and the Center is responsible for data anlysis. To date dozens of research monographs and special reports have been prepared by the staff. Responsibilities also include the preparation and distribution of data tapes for public use. Beginning in 1979, an additional cohort of 12,000 young men and women between the ages of 14 and 21 will be studied on an annual basis for the following five years. Again the Center will provide analysis and public use tapes for this cohort.

The Quality of Working Life Project is another ongoing study operated in conjunction with the cities of Springfield and Columbus, in an attempt to improve both the productivity and the meaningfulness of work for public employees in these two municipalities. Center staff serve as third party advisors, as well as researchers, to explore new techniques for attaining management-worker cooperation.

(continued on inside of back cover)